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AN ANALYSIS OF REFLECTIVE THOUGHT

IN replying to Mr. Buermeyer's criticism of my analysis of reflective thought[1] I suffer from somewhat the same embarrassment that affected him in writing it. He was handicapped by the fact that the analysis which he takes as the subject of his criticism was written for pedagogical purposes rather than for strictly logical ends. I am handicapped in replying by the fact that since Mr. Buermeyer states that he accepts the general instrumental position, and since he develops his own views incidentally in a criticism of portions of *How We Think,* I am not always quite sure of what his exact position would be were he writing to express his independent beliefs. In any case I shall ignore the statements of *How We Think,* and attempt to discuss the points made in Mr. Buermeyer's article on their own merits. It hardly seems to me that the original text naturally bears in all points the construction he puts upon it, but that is a minor matter, and if as acute a critic as Mr. Buermeyer misunderstood it, the text is hardly likely to have been unambiguous to others. So I am grateful to him for this opportunity.

The questions raised by Mr. Buermeyer concern the matter of steps or processes of thoughts. Starting from my analysis into (i) the occurrence of a problem, (ii) its specification, (iii) occurrence of a solving suggestion, or supposition, hypothesis, (iv) elaboration of suggestion, or reasoning, (v) experimental testing, he adduces reasons for holding that the reflective act can not be resolved into separate steps, and that, especially as thinking becomes more competent or scientific, the second, third and fourth "tend to fuse into one indissoluble act." Part of Mr. Buermeyer's contention I accept unreservedly, and am chagrined to find that I should have given any impression to the contrary. In speaking of "steps" it is perhaps natural to suppose that something chronological is intended, and from that it is presumably a natural conclusion that the steps are taken in a temporal sequence in the order taken above. Nothing of this sort, however, is intended. The analysis is formal, and indicates the logical "movements" involved in an act of critical thought. It is a matter of indifference which comes first. Not even the occurrence of a problem need come absolutely first in time. For a scientific man may reason and experiment for the express purpose of discovering a problem upon which to exercise inquiry. Were I writing at the present time and writing a complete statement, I should certainly emphasize the point that the main distinction between uncritical and critical or scientific thinking is that the latter strives to combine as far as possible into one act the functions of inferring and testing. The attempt represents an ideal or limit which can not be attained except in mathematics. But so far as the endeavor is concerned, I accept not merely Mr. Buermeyer's criticism of temporal separation of steps, but his conclusion regarding the fusion into one process of induction, deduction and experimental testing.

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[1] This JOURNAL, Vol. XVII, pp. 673-681.

Apparently this statement leaves no outstanding differences between Mr. Buermeyer and myself. However, the case is not so simple as this. Mr. Buermeyer says things which imply that in denying temporal separation he also denies any significant distinction of functions, while I still stand by the indispensable nature for logical analysis of these distinctions. Upon this point, however, the embarrassment to which I referred in the opening sentences comes into play. I am not sure just how far he means to go in the direction of logical as well as of psychological identification. In any case, he makes specific statements which point to obliteration of distinctions in logic between induction and deduction, and those statements will be used by me to clear up what seem to me significant logical distinctions. He refers indeed pretty continually to the functions of induction, deduction and experiment, in the course of showing their mutual involution. But whether this reference marks a recognition that induction, deduction and experiment are *logically* distinct from one another or whether it is a mere concession on Mr. Buermeyer's part to the exigencies of stating my own position for purposes of criticism, I am not sure.

This question is the important one, for it concerns the respective definitions of induction and deduction. My impression is that Mr. Buermeyer accepts the traditional statement of induction and deduction as logically movements respectively from the particular to the general and from the general to the particular. My chief concern is to modify this tradition. Induction I take to be a movement from facts to meaning; deduction a development of meanings, an exhibition of implications, while I hold that the *connection* between fact and meaning is made only by an *act* in the ordinary physical sense of the word act, that is, by experiment involving movement of the body and change in surrounding conditions. These are the points to which I hold, surrendering to Mr. Buermeyer as unclear and inadequate those portions of *How We Think* calculated to leave any other impression. There are of course points of contact between the traditional statement and that which I have just made. Facts, data, are logically speaking particulars, while meaning functions as a universal. But the traditional discussion takes either particular or universal or both for granted as given, while I am trying to account for them, and to account for them in terms of the reflective transformation of an experienced situation from a confused and uncertain state to a clear and coherent condition. In this process, data with their particularistic function present themselves when the situation is subjected to analytic observation; they represent the attempt to specify the problem. Suggested meanings present themselves as the means of restoring unity, coherence and consistency to the particulars. As such they have the function of universality. Experiment is the indicated application of meanings to the particulars to see what happens—to see whether the suggested unification can be carried out and maintained. Experiment has a two-fold function. From the side of suggested meanings it is a *test;* from the side of the otherwise fragmentary data, it supplies organization, system. There are various things in Mr. Buermeyer's criticism which indicate that he accepts the traditional idea of ready-made or given particulars and universals, data and meanings. At this point, we part company.

The explicit discussion of induction and deduction may conveniently begin with a reference to the fact that the foregoing statements assume but three functions; while the one quoted earlier includes four—leaving out the problem in respect to which there is no difference between us. This seeming discrepancy is due to the fact that the text of *How We Think,* with its practical pedagogic aim, was especially concerned with enforcing the difference between uncritical and critical thinking.[2] Now one of the most marked differences between poor thinking and good thinking is the former's premature acceptance and assertion of suggested meanings. One of the marks of controlled thinking is postponement of such acceptance. Consequently I inserted between the problem and the presentation of a suggestion the requirement of analytic examination of the "facts of the case.'' This ''step'' is not however different in critical thinking from the step of a tentative or hypothetical adoption of a suggestion. It marks the endeavor to *control* the form which a hypothetical meaning takes. The point as regards my view of induction can be made clearer by distinguishing between the inductive movement and induction as a critically executed function. Both terminate logically in an hypothesis. But in ordinary thinking no pains are taken to control the formation of the hypothesis. In critical or scientific inquiry great pains are taken to secure an accurate specification and collection of observed data as the means of control. This ''step'' is in my opinion the characteristic trait of *scientific* induction. There is no implication that suggestions do not arise until this step is taken. On the contrary suggestions swarm and press for acceptance. That is the very danger against which systematically executed analytic observation protects us.

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[2] The reader should not be misled by the fact that Mr. Buermeyer and I use the word "reasoning" differently. He uses it to express what I call critical or reflective thinking—thinking in its eulogistic sense. I prefer to confine it to "ratiocination" or rational discourse, the elaboration of implications.

Mill set out to lay down rules for induction which shall be as stringent as syllogistic rules were for Aristotelian deduction. Now as against this point of view it seems to me essential to maintain that the occurrence to the mind of explanatory ''causes'' is not a matter which can ever be subjected to stringent rules. The elements of individual capacity and of accident can never be excluded in the inductive operation. Huxley remarks that after reading Darwin and Wallace their theory of the origin of species seemed so obvious that he could only wonder how he and others who had access to the same facts should not have thought of such an obvious explanation. Here is the factor which can not be reduced to rule as Mill set out to reduce it. But on the other hand, a certain degree of regulation of occurrence of hypotheses does obviously occur. How? The reply of *How We Think*, to which I still hold, is that the analytic examination (extensive and intensive) of observed events supplies such control as is available. This process can be reduced to a considerable extent to rules. Mill's ''canons'' are not what he took them to be, but they are, especially the method of Difference and the Joint Method, statements of the way to conduct observation in order to secure the data which are *most likely* to render the suggestion of meaning (hypothesis, theory, ''cause'') relevant and fruitful—''most likely,'' other things being equal. There is no guarantee like that of the Aristotelian syllogism such as Mill aspired to.

Instead of ruling out Mill's contribution to the theory of induction I tried to place it where it belongs. This consideration supplies my answer to Mr. Buermeyer's remark that my account leaves me without the support of Mill's canons, and in general reduces induction to a mere matter of happy guessing. Just because the phase of happy guessing *can not be eliminated,* specification of the nature of the problem or analytic observation is of transcendent importance in induction. The performance of analytic observation of course involves experiment; it does not precede it. And it is guided by some idea or suggestion in most cases. Nevertheless there is a *logical* difference between experiment as resulting in data which affect the *formation* of a hypothesis and as *affecting* its acceptance. This is true even when the *same* experiment has as matter of fact both effects. Hence I can not accept Mr. Buermeyer's emendation that instead of defining scientific induction as the sum of processes by which formation of explanatory conceptions is *facilitated* (and regulated) it should be defined as those by which their *acceptance* is determined. Logically, we must distinguish the two results, although practically (as has been said) the ideal or limit of scientific method is that the same concrete procedure should effect both of them.

This is not a matter of splitting hairs. Practically it is essential in order that the hypothetic character of an explanatory conception may be adequately apprehended; and that the one thinking may make certain that a proving or testing experiment brings to light *other* facts than those which have led to its formation and (tentative) adoption. One has only to read current literature on spiritualism and interpretation of dreams, proceeding from men who have established scientific reputations in other fields, to note the practical importance of this discrimination. The history of science is full of similar cases—such as the elaboration of Weissmannism.

The theoretical bearing of insistence upon induction as connected with hypothesizing may be seen by examining Mr. Buermeyer's contention that it is "at least equally deductive in character." I do not differ from Mr. Buermeyer in holding that it will be adequately performed in the degree in which it utilizes deduction. But this statement implies that they are *logically* distinct, while Mr. Buermeyer goes to the extent of holding that they are identical. Deduction involves as he points out "the application of knowledge, of ideas already in hand" (p. 675). Now this he claims shows that forming a hypothesis is deductive, since it rests upon prior knowledge. "Only if we already have some information about a problematic situation, some experience of analogous situations, are we able to form a conjecture not wholly random" (p. 675). And again he speaks of the competent thinker as the one who "focuses upon the case in question all the funded results of the agent's past experience, the occurrence of analogies at once subtle and to the point" (p. 675). Without this deductive quality he holds that hypothesizing would be wholly casual and one man's theory likely to be as good as another's.

The necessity of information about the problematic situation is not only admitted, but insisted upon. It expresses precisely the need of regulating the formation of conceptions by analytic observation which has just been dwelt upon. But it is denied unqualifiedly that this information operates logically after the manner of a premise of deductive reasoning. If it did, any two men with the same technical competency would collect the same data and give their elements the same weight. Suggestion, the occurrence of a hypothetical meaning, is in last analysis a brute fact, alogical. It happens or it doesn't; a certain "idea" presents itself or it doesn't; some other conception holds the mind. (That a similar inductive jump actually takes place in all significant deductions, I do not question. But this does not identify their functions, that is respective uses, values, characteristic results.)

In reference to the use of reasoning from information derived from other and analogous cases Mr. Buermeyer seems to me guilty of a fallacy or confusion which I have elsewhere pointed out. (For example, *Democracy and Education*, p. 187.) There is a difference between saying that the suggestion of a hypothesis would not *occur* without prior experience and that it *rests* upon the use of prior experience. The former is a physical statement, the latter is a logical one. In fact men often have a suggestion occur to them without being aware of the prior experience which enabled the suggestion to spring up. Even in the cases when they are aware of an earlier experience which generated the suggestion they do not, if they are wise, place the suggesting experience and the suggested meaning in the relation of premise and conclusion, but only in that of suggester and suggested—"association of ideas" in common parlance. The logical question is whether the two situations *are* analogous. Knowledge exists and is used to suggest a hypothesis. But the question for *present knowledge* is whether the old case or rule is or is not applicable to the new one. Many of our common errors come from assuming that what is known in some cases is also knowledge for the case in hand. This kind of subsumption is the essence of all dogmatism. Deductive reliance upon old knowledge, that is putting the old case and the conception it suggests in the new case in the relation of logical premise and conclusion, is precisely the thing against which the inductive function has to safeguard us. That a trained man can rely upon old knowledge more confidently than an untrained one is a fact. But the logically trained man still makes a distinction between old knowledge as a source of suggestions and as a deductive premise for a conclusion. The analytic inspection upon which I have laid such emphasis as the crucial thing in induction has to extend not merely to the present problematic situation but to the prior situations from which a would-be rule or conclusion springs up. Only in this way, can we safeguard the acceptance of a suggestion by determining the degree of similiarity which exists between the two cases. Mr. Buermeyer refers to Newton's inference as to gravitation. Well, why not refer also to his inference as to light? It was the same man working with the same instrumentalities. In one case, the supposition of analogy with prior experiences has been confirmed. In the other case, it hasn't been. In short, the *hypothetical* character of inductive inference lies precisely in the supposition of analogy between the present problematic cases and other assured cases. This is the point at issue. There is no paradox in the fact that what is knowledge in one context is hypothesis or even error in another. But it is a fact which precludes that acceptance of prior knowledge as a deductive premise upon which Mr. Buermeyer's argument depends.

Upon the importance in induction of a plurality of competing hypotheses and of the importance of elimination, of exclusion, of all but one, I am glad to find myself wholly at one with Mr. Buermeyer, and also to accept the explanation which he gives (on p. 676). I should also agree with Mr. Moore that the assumption of a plurality of hypotheses as applicable (that is, as worth trying to apply) to the same set of facts is of the essence of skepticism. But I should add two qualifications. Such an assumption is itself involved in the hypotheses; or it is itself a hypothesis, namely, that each one of a number of hypotheses having *prima facie* claims is worth developing and examining. And I should add also that skepticism about the categorical value of an inductive inference is a prerequisite of good thinking. Part of the worth of competing hypotheses and of the method of successive elisions is that it fosters precisely his healthy skepticism.

But I can not see in acceptance of the importance of the method of elimination anything which militates against my analysis. I can see in it several things which seem to go against Mr. Buermeyer's. For example, since all competing hypotheses are equally suggested by the problematic situation in conjunction with prior analogous situations, they are *not* deductive conclusions from identical premises but are suggestions springing up from different sources. In the latter case each may need deductive amplification and be worth experimental testing. An assertion that incompatible hypotheses are deductively grounded in identical premises is, it seems to me, skepticism of the most nihilistic kind; it destroys the very possibility of any valid deduction. Mr. Moore's statement from this point of view is quite moderate. But it implies no depreciation of plurality of hypotheses and a process of selection among them by a method of elimination. On the contrary. If one hypothesis is good because it starts one train of deductive implications and initiates one set of experiments, several are better because they extend the operation. In any complicated case, it would be practically impossible to arrive at a sound conclusion save as various deductive systems were compared and the results of different experiments used to check one another.

Mr. Buermeyer's criticism applies also to the treatment of deduction as the development of a suggested meaning through tracing its implications, that is, its logical connections with other meanings or conceptions. His discussion of this point is, however, comparatively brief, and mine must also be. He speaks of deduction in modern logic as "a general theory of types of order, or implication" (p. 676). I see nothing in this conception contrary to my point of view. Because ordinary reasoning constantly employs implicatory relationships in order to expand or elaborate a suggested meaning, there is every reason why an abstract theory of implicatory relationships should develop. Many of those who have engaged in the development of this logic would deny their ultimate instrumental or methodological character. But Mr. Buermeyer's general acceptance of the instrumental logic does not suggest any such disposition on his part. He says, however, that while the development of a content of a hypothesis uses these types of order, it can not be identified with them, since it is partial or selective. Precisely. Just as induction employs selected matter-of-fact information in forming a hypothesis, so deduction employs a system of previously formed conceptions or meanings or relations. It has to pick out and adapt for the purpose in hand, which is set by the problem. This selection is inductive because it involves forming an hypothesis regarding their value or applicability for the purpose in hand. Their established position within the general system of types of implication does not guarantee their appropriateness in the given context any more than the assured place of information in one context guarantees its use in a new problematic situation.

Up to this point I but repeat my agreement with Mr. Buermeyer so far as the use of induction in deduction is concerned. It would also be admitted or claimed that deduction occurs for the sake of experimental testing. More than this, there is constant testing *during* the deductive process; mentally conceptions are tried together to see how they fit for the purpose in hand. Otherwise, as Mr. Buermeyer acutely remarks, deduction would become like the calculations of an adding machine (p. 680).[3] But this dependence upon and use of induction does not, that I can see, affect the statement that the *function* of deduction, or deduction logically viewed, consists in elaboration of a meaning of which has first presented itself in a crude, undeveloped form, and which is accepted not finally but as worth being made the base of development.[4] In fact, it is not easy to tell whether or not Mr. Buermeyer denies this point.

In conclusion, one ultimate point at issue may be said to turn upon the relation of act (or process) and function, or as Mr. Buermeyer points out the relation of the psychological and logical. I concede wholly to Mr. Buermeyer the contention that in the degree in which any job of thinking is well done, experimenting and deduction are *involved* in induction—and so on all the way around. But I deny that this factual involvement means logical identification. This denial seems to Mr. Buermeyer to imply going back upon that belief in the intimate relation of psychology and logic which is the essence of the pragmatic or instrumental logic. I do not think so. One might contend that a *science* of physiology depended upon ability to detect chemical *processes* and trace their workings with respect to all the functions of the organism. Would this cancel the distinction between breathing and circulation, even if it were shown that they not merely depend upon each other but that fundamentally similar chemical reactions were found in both? Function is not a separate process: to suppose that it is, seems to me the error of all abstractionism and absolutism. It is, however, a distinctive matter, for it concerns the results of processes. A man may go from New York to Chicago and from Chicago to New York upon the same tracks, in the same car and with the same engineer. But the purpose, the outcome is different. So is it with induction and deduction. The more we become aware of the identity of psychological process, the more the difference in function becomes significant. To ignore this distinctiveness of function because of the unity of act seems to me to deliver the cause of concrete, psychological thinking into the hands of the enemy—those who assert that all true reasoning is deductive and that induction is a mere psychological preliminary, important only in the biography of the individual thinker. I wish that Mr. Buermeyer would turn his well-loaded guns upon that camp; and in conclusion I again thank him for the opportunity to make clearer points which in *How We Think* were doubtless left in regrettable obscurity.

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[3] I take it that Mr. Buermeyer would probably agree with me that the same thing holds of the general theory of types of order or implications which the new school substitutes for the old syllogism. As a matter of fact, they also involve induction.

[4] If Mr. Buermeyer had carried his criticism to the point of stating that conclusions appear in every phase of reflective thinking I should also have agreed with him. It is far from being true (as a chronological interpretation of my analysis would infer) that conclusion is postponed till the problem is solved. We accept or adopt at every point. The difference is in the conditions and purpose of the acceptance, as is suggested above regarding acceptance of a hypothesis.

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THE NERVOUS SYSTEM, PSYCHOLOGICAL FACT OR FICTION?

SO habituated have we become to the inferior position which psychology occupies among the sciences that we have become accustomed to excuse its deficiencies rather than to understand and correct them. Hence, the tradition continues that psychology deals with vague and futile materials, not natural facts which can be described and referred to valid laws. Indeed, it is only with extreme reserve that one grants psychology a place among the natural sciences at all. Of course it is the psychologist himself who is responsible for this situation for he is only very slowly prying loose the facts of his domain from the metaphysical incrustations in which the centuries have confined them.

The psychologist's handling of the nervous system is an excellent case in point. The nervous system, originally brought into psychology as a means of concretizing and interpreting the diaphanous and fleeting states of mind, has not yet been provided with its proper place as a component factor in a complex psychological act. Instead, it is mainly used as a scheme wherewith to handle the elusive knowings or awarenesses which are still all too prominent in psychological writings. Although the nervous system is made to do heavy duty in psychology, as is manifest from even the slightest examination of psychological literature, it is only in the case of reflexes and similar actions that it serves in any sense as a descriptive factor. In practically all other cases the nervous system is used in psychology merely as an explanatory agent. In the present paper an attempt is made to investigate the neural conceptions prevalent in psychology with the hope that we can thereby suggest what is factual and what fictitious in these conceptions.

Unfortunately, at present it happens to be true that in general, whether psychologists use the nervous system as a descriptive fact as in the study of reflex action, or as an explanatory instrument in other cases, the results so far as psychology is concerned are